Dr Radcliff (2)
with kind regard
from John Murr

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& India

REMARKS

ON

M. PETTENKOFER'S VIEWS ON CHOLERA IN INDIA.

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I HAVE had an opportunity of seeing a translation, by Dr. D. Cuningham, of Professor Max von Pettenkofer's last book, "On the Diffusion of Cholera in India", in the *Calcutta Annals of Medical Science*, No. xxix. Any work on cholera, proceeding from the pen of this celebrated professor, deserves the most serious attention. The present calls for especial notice, as, after a most elaborate and searching inquiry into the reports on this disease that have lately appeared in India, he arrives at the conclusion that cholera is not contagious, and advances the opinion that quarantine is not only most vexatious and expensive, but also useless. As this is a practical question, it is highly desirable that the soundness of the opinion should be scrutinised, lest the powerful influence of the author's name should cause the neglect of precautions indicated to restrain the dissemination of this most fatal disease. Precision in the definition of all the terms employed is essential to philosophical discussion. In the present argument the word "contagion" appears to be understood in a different sense by M. Pettenkofer and by myself. That in which I employed it in my Report on Cholera in India is the same that has been assigned to it by some of the most eminent medical writers in England—viz., the property of transmission or communication of a specific disease directly or indirectly from a sick to a healthy person. The table in my Report, criticised by the professor, shows that this was my meaning, the various columns under the heading "Contagion" indicating the different channels through which the disease might be communicated: this is also implied by the recommendation of isolation of the infected, and employment of special hospitals for the sick under the heading "Precautionary Treatment".

In order to estimate the force of M. Pettenkofer's argument, that cholera is not contagious, it is essential that he also should define the meaning he attaches to the term "contagion", that it may be seen in what respect his views differ from mine. In one table (No. vii) I abstracted the opinions expressed or implied in the answers which I received from the officers of the British and Indian medical services to the queries which had been circulated under instructions from the Indian Government. It was on seeing the facility of thus recording opinions, and the economy of space and time both to the writer and to the compiler, that the formula of this table was subsequently circulated, with a request that the answer should be returned by filling it up, and be accompanied by memoranda on any point worthy of notice by the officer who replied. Numerous extracts from these answers are given in the appendix to the Report, and advantage was taken of the receipt of several of these abstracts, while the Report was passing through the press, to raise the numbers in the table from 481 to 505. I have now the register of each individual name, with the detailed opinions of which Table No. vii is an abstract. This table shows the opinions of five hundred and five intelligent and experienced medical officers on the most important questions in the history of the disease and its treatment, and more especially on its contagious nature. Their opinions on this point range in the affirmative from 99.6 per cent. who decide on the necessity of isolation, to 90 per cent. who agree in the communicability of the disease through the person. The ratios vary in the other channels from 94 to 99 per cent. of those who give a decided opinion. The professor, being himself a non-contagionist, considers these opinions as opposing instead of concurring with each other, and says that as "no" is a more decided opinion than "yes", it would be more correct to count the negatives as determining the opinions of the Indian service. To me this reasoning is incomprehensible, the affirmatives being so numerous as to attain almost to unanimity. Neither do I understand his subsequent statement that the contagionist view now only maintains a struggling existence in India, unless he wish us to conclude that the members of the profession there have meant "no" when they said "yes".

I concur with M. Pettenkofer in thinking that human intercourse is the main channel through which cholera is diffused over the world. We only differ, first, as to the development of the germ (or active agent, or poison) in the human body; and secondly, as to the necessity of the germ passing through certain changes in its existence outside the body before it can excite the disease. M. Pettenkofer considers that it can only develope and multiply in the subsoil-water, and not in the human body. I consider that it does develope and multiply in the body, and that it may be transferred, unchanged, from a sick to a healthy body, and there be developed and multiplied; but that it is more generally developed and diffused outside the body in the locality, in various extraneous media, such as impure air, or water, or moist soil, and that the influence of the subsoil-water in the latter case is often great, but not essential. If the professor's views be correct, so would be his conclusion that quarantine is vexatious, expensive, and useless. Should my views be correct, the arrest of the dissemination of this most fatal disease by human intercourse should be attempted, even though quarantine is vexatious and expensive. That the means hitherto employed under this name have only been partially successful, and have not been effectual in all situations in attaining the desired result, is a lamentable fact. That they are practically inapplicable in some circumstances is apparent; but it is equally true that the extension of the disease has been restrained by them in many instances, as shown in the Indian reports in reference to gaols and cantonments. Instead of abolishing quarantine, as recommended by the professor, I consider it the duty of government to improve the laws now in force. The foundation of such improvement should rest on increased knowledge of the history of the poison, when outside the body, in reference to such influences as promote or retard its development, or destroy it entirely. In reference to M. Pettenkofer's opinion of its development in the subsoil-water only, it is highly probable that it exerts a decided influence in this direction; but it is exceedingly difficult to isolate this from concomitant influences generally associated with its development, particularly season; besides, there is always subsoil-water where there is land, though sometimes it lies three hundred feet beneath the surface, and is not likely to exercise much influence on a passing disease. The observations that have lately been instituted in India have not hitherto shown any marked connection between the diffusion of cholera and the change in the level of the subsoil-water; and the careful investigations of the sanitary commissioner of the central provinces, Dr. Townsend, tend towards an opposite conclusion.

The history of the disease on board emigrant ships from England to America, and coolie ships from India to the Mauritius and West Indies, appears to me incompatible with any connection with the subsoil-water. M. Pettenkofer's explanation, that the poison was imbibed from the soil previously to embarkation by all those who were afterwards attacked, and that it remained dormant, or incubating for unusual periods, and then, simultaneously developing, broke out in a large number of cases, is not impossible; though it is more probable that the disease was kept up by succession of slight cases, not proceeding beyond the stage of diarrhœa till the bad hygienic state of the vessels brought other influences to bear in rendering the disease more severe, and causing it to assume a more intense form and spread more rapidly. Or again, the disease may have originated in the ship from the exposure of those on board to infected clothing. M. Pettenkofer's idea of protracted incubation requires that the ship should have sailed from a tainted locality, which could generally be said of all Indian ships; but in the case of those emigrant ships which did not sail from tainted ports, there is evidence that part of their passengers had lately come from places where cholera existed; and these suffered first, but the disease spread most virulently amongst the other emigrants who had not come from a tainted locality. The professor considers that the subsoil-water is the paramount agent in the dissemination of the disease. I am not aware on what authority he states that the profession in India assume the communication through the air of a soil-born miasm as described by Dr. Bryden. If by this be meant that human beings could not communicate over the world without the air, doubtless he is correct in his view, but not in saying that the direction of the wind influences the spread of the disease. If M. Pettenkofer means that cholera does not spread in the earth where there is no subsoil-water, he would not be far wrong; but till he can show an instance where the disease spread from one country to another through the subsoil-water where there was not human intercourse, the force of his objection to quarantine has no more power than that of Dr. Bryden's theory of the wind.

It was thought in India that the question of contagion was settled by the history of the Hurdwar epidemic. The daily progress of the disease was recorded by all the civil surgeons of the upper provinces and the Punjab, by orders transmitted from me three days after the disease had appeared at Hurdwar. Their reports from the various districts showed that the first cases of cholera were in pilgrims, but that the disease spread from them to the inhabitants, causing great mortality, amounting during the first two months of the epidemic to 30,000. In the civil divisions of Meerut, Rohilcund, Umballa, Delhi, Hissar, and Agra, there died in the month of April 6,932, in May 11,072, in June 9,269 ; and in the Punjab there died in the month of April 703, in May 2,619, and in June 5,231. These are not the ordinary seasonal months for cholera in these parts of India. The total of deaths in the European hospitals in these divisions of the North-West Provinces during the previous fifty-five years, deducting the years of the mutiny (1857 and 1858), was in April 43, in May 54 ; and in June 59 ; and in the Punjab for the previous twenty years, in April 5, in May 10, in June 7. [I quote these numbers from the European Hospitals, because Dr. Bryden considers them typical of the disease in the country.] This gives an annual average of one death for each month in the North-West Provinces, and one death in three months for the Punjab. The seasonal months for these longitudes, as indicated in the returns now quoted, are July, August, and September. There was, therefore, no seasonal complication at the time of this epidemic ; and as it was an ordinary year, there was no unusual variation of the subsoil-water, so these elements may be eliminated from the causes which probably induced it.

M. Pettenkofer considers that the pilgrims imbibed the poison at Hurdwar, and that it lay incubating for various periods, which in some instances—as in those who went to Cashmere, Peshawur, and Jellalabad—must have extended from one to two months, which is most improbable. I consider that the disease was kept up and carried on by fresh pilgrims encamping on ground vitiated by parties who preceded them on the road. The inference is, that the disease which affected the inhabitants was communicated to them by the pilgrims, as it was proved to be synchronous with their arrival. This is the general opinion of the medical officers who had the opportunity of tracing the disease through their districts. The incidents detailed in the report on the Hurdwar epidemic show the various channels through which the poison was in some instances transmitted, and throw great light on the obscure theories engendered by imperfect knowledge. The wind theory is inadequate to account for this epidemic, as it would have required a wind to blow at once in all directions of the compass ; neither can it be explained by the seasonal theory, which in that locality should have been July and August, instead of April and May. The subsoil-water theory also fails to account for it, as it was a normal year, and the subsoil-water varied from five feet to three hundred feet from the surface of the ground without appreciably influencing the progress of the disease. As this official report cannot be generally known, I may quote two instances, which I consider valuable typical cases of cholera being communicable through water.

1. From the Report of W. G. Blyth, Esq., Deputy Commissioner of Montgomery, between Lahore and Mooltan. A pilgrim was taken ill of cholera at noon on the 28th April, at a well in the village of Jooghkullun: he died next day, and his soiled clothes were washed in an adjoining pool. Other parties who afterwards visited the well and the pool for water and ablution caught the infection ; the disease broke out on the 30th, and up to the 15th May fifty-three were attacked, of whom twenty-seven died. There had been no cholera in this village for the previous twenty years.

2. From Dr. Gardner, Civil Surgeon of Bijnour. At the village of Bisakee, two men who had returned from Hurdwar Fair died of cholera on the 30th April, and their clothes were not burned according to orders given, but were washed in a pond near the village, the water of which was used for domestic purposes. On the 1st and 2nd May, in this vil-

lage, sixteen attacks occurred. There had been no cases previously during the year.

The great difference in the mortality among the European and native troops, shown in Dr. Bryden's tables to be among the European troops 53.8 per 1,000, among the native troops 4.11, is attributed by M. Pettenkofer to race, and supposed to be unconnected with latrines, as believed by me. He has omitted to notice Dr. Bryden's returns of the mortality among the native prisoners. I most fully concur in the high eulogium passed on Dr. Bryden's zeal and energy, and in the professor's estimation of the great value of the statistical tables which he has drawn up. They extend from 1826, and I have tested their accuracy and found them correct. In addition to these, I have returns of the mortality in the European troops in the Bengal Presidency since 1814. I have also returns from the civil population since 1867 (with the exception of Bengal proper). These latter returns are less reliable than those from the military and civil hospitals, and extend only over a short period. From these returns, I have formed four tables showing the monthly mortality in each class, and also the relative sickness during the seasonal period of cholera. As this varies in so large an extent of country, I have divided it into two sections, that lying to the east and that to the west of Allahabad. From these tables, it appears that the mortality among the native prisoners was higher than among the European troops in the stations east of Allahabad, and that in the stations west of Allahabad, though lower than amongst the European troops, it was much higher than amongst the native troops or inhabitants of the country. The seasonal period for cholera in the country east of Allahabad is the three months, March, April, and May, and in that to the west, July, August, and September. The following shows the relative mortality.

	EAST.	WEST.
European troops	58.5	66.5
Native troops	13.9	4.6
Native prisoners	64.8	24.0
Native population	——	1.4

The mortality during the cold season is trifling in all classes. This table shows a striking coincidence in the high mortality among the European troops and native prisoners, and in the low mortality among the native troops and civil population. The only points in which there is a resemblance in the circumstances of the European troops and native prisoners which were not common to the native troops and inhabitants, were that the former lived congregated in large rooms or barracks and used public latrines ; doubtless both these had an influence on the result. The native troops and inhabitants live in small rooms and have no public latrines. Race has here no appreciable influence; and if the effect of personal contact be excluded (in accordance with M. Pettenkofer's views) the influence of the latrines alone remains as the probable cause of the higher mortality. As the atmosphere and subsoil-water in cantonments are the same for the troops, both European and native, and generally in this flat alluvial country for the jails and surrounding districts also, they must act equally on all.

In section 10, M. Pettenkofer treats of change of locality as carried out in the removal of troops in India, and arrives at the conclusion —"That, on the whole, it is manifest that every change of locality is not *per se* of benefit, but the result is dependent on how the abandoned locality is related to the local cholera-generating process at the time of departure, or how soon or late the locality is abandoned, and in the same way on the condition of the place which is had recourse to, for it is possible to pass from a better to a worse in such a change of locality." This sounds like an echo of the opinions of Dr. Bryden, who is opposed to removal and a non-contagionist, and who ignores the danger of contracting the disease from the person or place vitiated by the person. I differ in opinion from both, and consider that removal from a tainted locality is *per se* a benefit; and in this I am supported by 99 per cent. of the medical officers in India, who are of opinion that there is danger in a place where cholera exists, and recommend removal from it. The professor is under a misapprehension in supposing that the sick accompany the removal. On the contrary, the sick are left in cantonments on moving out, and only those in whom the poison is in a state of incubation, or the stage of *malaise*, where it is not recognised, accompany the detachment. When the disease subsequently advances to the more active stages in camp, the men are treated in separate hospitals. It is my opinion that a considerable portion of the detachment often have the disease in this stage which is thrown off through the natural channels without advancing even to diarrhœa, and that in this form it is capable of contaminating the ground. The guiding principle of this important measure is to avoid danger from those infected by the disease and the locality vitiated by them. This is carried into effect by removing the men into tents and encamping on high, dry, and well-drained ground, avoiding the vicinity of swamps or streams. Care should be taken to avoid exposure to the sun, and over-fatigue from long marches, and the camp should never be formed in any place where cholera has lately existed. Cholera cases appearing in camp should be treated in special hospitals. The result of the practice of removal since it was introduced in 1860 has been considered highly satisfactory. Out of 80 removals of troops and prisoners, which took place in 1867, in 34 instances there was no case at all after change into camp ; in 7 instances there were no cases after the first day ; and in 4 instances there were none after the second day. It thus appears that in more than half these removals the disease was left behind. Removal is a most delicate, expensive, and disagreeable operation, especially in the hot and rainy seasons in India, and only resorted to as the lesser of two evils. If judiciously directed, it removes panic and inspires hope, and also checks the progress of the epidemic. If injudiciously managed, it is attended with danger, and sometimes followed by most disastrous results, as mentioned by M. Pettenkofer in the case of the 104th Regiment at Peshawur, in 1869. I shall repeat the history of this case, as it illustrates the views which I have expressed regarding the danger of encamping on ground that has been vitiated by previous parties suffering from cholera. The left wing of the 104th, after the occurrence of five cases of cholera, removed on the 13th September to Chum Kunnie, and marched next day *en route* to Cheraat, and they only lost two more cases, which is a highly favourable result of removal. The remainder of the regiment continued to suffer daily from cholera till the 17th, when it also marched to the same ground at Chum Kunnie, and followed the same route. It had 3 cases on that day, 15 the next, and 27 the following day, and the disease continued amongst them for ten days longer. The only way to account for this different result is by the fact that the ground had been vitiated by the left wing before the passage of the right wing over it. Strong theoretical views blinded those who afterwards endeavoured to trace the cause of this disaster, and prevented their detecting the danger that lay in the infected ground. The necessity of prompt removal is deemed so urgent that it is strictly enforced by the orders of government. It is made imperative after the occurrence of more than two fatal cases in one barrack, and is independent of the opinion of the local military authorities or theoretical medical views. Recent experience has shown the soundness and necessity of these stringent orders, and the disastrous result of neglecting them.

So long as philosophical musings on theoretical questions are the relaxation of zealous students, those of M. Pettenkofer on the possible or probable changes of the cholera-germ in passing through the subsoil stage of its transition existence must give pleasure to all true investigators of disease. There is an important element in the human organisation, which has been omitted or overlooked by the professor and some other recent writers, which has unquestionably a decided influence on the propagation of this disease and its action on the human body. This influence was formerly called the *vis medicatrix naturæ*—a vital power by which inanimate substances are converted to the use of the body, which controls the natural processes of decay called fermentation and putrefaction, and without which both the food and the body itself would progress through these natural courses to decay. It may be said that this is speaking vaguely, but it leads us to consider how these processes are regulated, and by what means they are conducted in the healthy body, and how their absence or removal would influence the natural changes in food or poisonous bodies introduced into the system. It would be well to follow up the investigation by searching out by what means these agencies in the body are generated, and how they act in preventing the entrance of the cholera poison into the body, or in modifying its action when there, and expelling or removing it from the system. This forms one of the most important points under discussion, more important even than a knowledge of the history of the poison outside the body, to which attention has been chiefly directed by the professor.

The sanitary inquiries of recent times have been principally directed to the history of the dissemination of the disease Such investigations are of great value, but they are merely preliminary to the main question of curing the disease when the poison has entered the system. In this branch of the inquiry the opinion of so profound a thinker as M. Pettenkofer would be of great value, and to this subject I trust he may turn his attention.